

## **Exhibit 4**

**UNITED STATES DISTRICT COURT  
FOR THE SOUTHERN DISTRICT OF NEW YORK**

**NATIONAL ASSOCIATION FOR THE  
ADVANCEMENT OF COLORED PEOPLE,  
SPRING VALLEY BRANCH, *et al.*,**

**Plaintiffs,**

**v.**

**EAST RAMAPO CENTRAL SCHOOL  
DISTRICT, *et al.*,**

**Defendants.**

**ECF CASE**

**Case No. 7:17-cv-08943**

**DISTRICT JUDGE  
CATHY SEIBEL**

**MAGISTRATE JUDGE  
JUDITH C. MCCARTHY**

**SUR-REBUTTAL REPORT OF DR. JOHN ALFORD, PhD.**

In their expert rebuttal report dated November 16, 2018, Collingwood and Barreto offer several responses to my October 26, 2018 report. While Collingwood and Barreto raise several specific criticisms, there is a common thread that flows throughout their report: Collingwood and Barreto embrace novel approaches, and argue that those approaches are superior to the tried and true methods that political scientists—including Barreto—have used for decades. A deeper analysis reveals that Collingwood and Barreto only rely on novel approaches when evidence from the traditional approach is damaging to the plaintiffs’ position. Each “innovation” is discussed below.

**Absence of any Focus on Minority-Contested Contests**

Election analysis in voting rights cases typically focuses on racially contested elections when they exist, as they do here. As detailed in my reports, there is no indication that the race of

the candidates plays any role in the behavior of voters in the elections analyzed in this case. This is not helpful to the plaintiffs' position. Departing from their past practice, Collingwood and Barreto decided in this case not to use racially contested elections as a starting point for their analysis of the elections, and in fact never consider it at all.

### **BISG**

The CVAP based EI RxC analysis that breaks out Black and Hispanic support separately does not show minority cohesion. Collingwood and Barreto report each of the *other* CVAP based estimates, omitting only the ultimate Black and Hispanic breakout. This is where they decide that a novel approach – BISG – is needed. They do not use BISG throughout, and continue to rely on CVAP based estimates of turnout and of white versus non-white voting behavior in their report and rebuttal report.

### **Election Algebra**

Consideration of the Gingles threshold factors typically proceeds in order from one to three. The election analysis first seeks to determine if minority voters vote cohesively, and then moves on to see if white voters are cohesive in opposition to minority-preferred candidates and vote sufficiently as a bloc to usually defeat those candidates. Where plaintiffs seek, as they do here, to combine two distinct racial and ethnic groups, they must demonstrate that the groups are sufficiently cohesive to justify that combination. Rather than follow that approach, Collingwood and Barreto reverse the process and focus first on *Gingles* prong three, and do so with the first ever use of 'election algebra' in a Voting Rights Act case.

### **Relaxing Traditional Social Science Levels of Statistical Significance**

The social science standard for tests of statistical significance is the .05 level or the equivalent 95% confidence interval. Applying that standard is problematic for the plaintiffs'

position, even in the novel BISG based analysis. Rather than follow the standard, Collingwood and Barreto argue here for a focus on what is more likely and what is less likely. This is not what motivated the selection of, or the continued use of, the 95% confidence interval in the social sciences, and is not the approach that is typical of published research.

### **Eliminating Consideration of Socio-economic Disparities in the Senate Factors**

Senate factor five concerns “the extent to which minority group members bear the effects of discrimination in areas such as education, employment, and health, which hinder their ability to participate effectively in the political process.”<sup>1</sup> In this case it is the majority white voters that are disadvantaged on many of the traditional census measures of socioeconomic conditions. Collingwood and Barreto chose not to consider this in their report, even as regards how it might interact with other Senate factors.

Throughout their analysis Barreto and Collingwood utilize their purportedly “innovative” approaches to avoid dealing with the damaging conclusions generated by using standard social science methods. If the EI RxC analysis using CVAP data is unhelpful to Plaintiffs, then Barreto and Collingwood discard it and use BISG or Election Algebra instead. If the standard social science test for statistical significance shows that their estimates are not statistically significant, then Barreto and Collingwood argue for a different standard of statistical significance. If the data on the socioeconomic status of Blacks and Whites in the District fails to conform to national trends in ways that are unhelpful to Plaintiffs’ case, then Barreto and Collingwood simply ignore that data. And if the evidence shows that the white population of the District supports and elects Black, White, and Hispanic candidates without variation in the degree of support according to

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<sup>1</sup> S. Rep. No. 97–417, at 29.

the race of the candidates, then Barreto and Collingwood ignore the race of the candidates entirely.

### **Policy Polarization versus Racial Polarization**

The central conclusion of my analysis of elections in the case is that the stable levels of support for candidates across these elections is a function of a long-standing *policy* polarization over details of private/public school support, and not a pattern of *racially* polarized voting. As I explain on pages 24–25 of my Report:

*Going down the whole line of contests from 2017 to 2013 it is clear that there is very little variation in the share of the White vote going to the White preferred candidate, and what modest variation there is does not appear to follow any pattern that is related to the race of any of the candidates. Looking down the list of CVAP white estimates shows that the White preferred candidates in 2017 got 86% in a White vs White contest, 86% in a White vs Black contest, and 87% in a White vs Latino contest. In 2016, the White preferred candidates got 85% in a Black vs White contest, 85% in a Black vs Black contest, and 84% in a White vs Latino contest. In 2015 the White preferred candidates got 82% in a White vs Black contest, 82% in a White vs Latino contest, and 78% in a Latino vs White contest. In 2013 the White preferred candidates got 83% in a Latino vs. Black contest, 80% in a Black vs Black contest, and 80% in a Black vs Black contest.*

*Again, this same lack of variation in the share of the White vote going to the White preferred candidate is evident in Collingwood and Barreto's preferred BISG estimates. Looking down the list of BISG White estimates, the White preferred candidates in 2017 got 73% in a White vs White contest, 74% in a White vs Black contest, and 77% in a White vs Latino contest. In 2016, the White preferred candidates got 75% in a Black vs White contest, 74% in a Black vs. Black contest, and 72% in a White vs Latino contest. In 2013, the White preferred candidates got 68% in a Latino vs. Black contest, 67% in a Black vs. Black contest, and 66% in a Black vs. Black contest.*

Departing from past practice in voting rights cases, Collingwood and Barreto offer no analysis that seeks to assess the levels of support of whites or minorities for Black or Hispanic

candidates. In previous cases, the race/ethnicity of candidates has been the key selection criteria used to determine which elections to analyze. Indeed, even in Barreto’s prominent 2007 APSR article on Latino voting behavior, the central focus of the research is on the impact of Latino candidates on the behavior of voters.<sup>2</sup>

Here, in contrast to the findings in that article, the ethnicity of the candidates does not appear to matter, and Collingwood and Barreto simply avoid the entire issue of candidate race/ethnicity as it relates to the behavior of the voters both in their initial reports and in their rebuttal report.

### **CVAP versus BISG**

The EI RxC analysis using CVAP data does not support a finding of Black and Hispanic voter cohesion for purposes of the Gingles precondition 2 analysis. Collingwood and Barreto, despite providing extensive EI and EI RxC analysis of voting patterns for white vs non-white, and for turnout using CVAP, conspicuously avoid producing any EI RxC analysis using CVAP that estimates the voting behavior of Black and Hispanic voters separately. Such analysis is crucial when plaintiffs seek to combine two otherwise distinct minority groups,<sup>3</sup> and Collingwood and Barreto expend considerable effort to substitute a novel BISG based EI RxC analysis for the standard CVAP approach.<sup>4</sup> My concerns with the BISG (and related Catalist) approach as used here is already covered in my previous report.<sup>5</sup> While Collingwood and Barreto do not dispute the specific CVAP EI RxC results provided in my report, they do offer several general comments on the CVAP based analysis that I will address here.

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<sup>2</sup> Matt A. Barreto, ¡Si Se Puede! *Latino Candidates and the Mobilization of Latino Voters*, 101 American Political Science Review 425 (2007).

<sup>3</sup> See Alford Report at 10 & n.10.

<sup>4</sup> See Collingwood & Barreto Rebuttal Report (“B&C Reply”) at 6–11.

<sup>5</sup> See Alford Report at 12–14, 21–22.

In their CVAP discussion in their rebuttal, Collingwood and Barreto begin by asserting that I have acknowledged earlier in this case that CVAP-based analyses are not reliable. On pages 6–7 of their rebuttal report, they assert that:

*Dr. Alford agrees that CVAP estimates of Black and Latino voters are not as reliable as using data on the actual people who voted. In a prior report, labeled Exhibit B, Dr. Alford writes: “The problem with [using CVAP to analyze racially polarized voting] is that it assumes, without justification, that racial groups vote in proportion to their size—i.e., that if Blacks comprise 30% of the voting age population, then 30% of the votes cast in the election will be cast by Black voters. In fact, studies have disproven that assumption.” He continues in the next paragraph of his earlier report that the problems and solutions to the CVAP issue are outlined in a published research study that we contributed to: “The problems associated with using CVAP as a proxy for turnout are well recognized in the literature. The problem, as well as potential solutions, are described in considerable detail in Bernard Grofman & Matt A. Barreto’s A Reply to Zax’s (2002) Critique of Grofman and Migalski.”*

Collingwood and Barreto have seriously mischaracterized my position. In that report I am specifically discussing the way in which Plaintiffs’ first expert, Dr. Cole, used CVAP in an analysis that failed to incorporate an estimate of, and adjustment for, turnout difference.<sup>6</sup> In that discussion, I mention two widely used alternative methods that are appropriate, both of which are also mentioned in the Grofman and Barreto article that is cited,<sup>7</sup> and suggest that Dr. Cole should have used one of those techniques:

*The most widely used technique for dealing with this issue is some form of double equation regression or double equation EI as discussed and advocated in detail in the Grofman and Barreto article that Dr. Cole cites in his rebuttal. Dr. Cole could also have used a more modern true RxC approach for his EI estimation (that is, third-generation EI). In a true RxC approach, like the one I used in this case, turnout differences are captured in the model by including non-vote, along with the various candidates, in the possible choices a voter might make.<sup>8</sup>*

<sup>6</sup> See Alford Affidavit of Direct Examination at 7.

<sup>7</sup> *Id.* (citing Bernard Grofman & Matt A. Barreto, *A Reply to Zax’s (2002) Critique of Grofman and Migalski*, 37 Sociological Methods & Research 599 (2009) (“A Reply to Zax”)).

<sup>8</sup> Alford Affidavit of Direct Examination at 7–8.

My concerns about Dr. Cole’s use of CVAP was precisely his failure to utilize either of these appropriate methodologies, *not*, as implied by Collingwood and Barreto, a concern about the use of CVAP itself. Barreto is a co-author of this article, and the article is advocating for methodologies designed to allow the use of VAP (“voting age population”), or preferably CVAP, while making appropriate empirically based adjustments for turnout.<sup>9</sup> This is exactly what the EI RxC approach in both my reports, and in Collingwood and Barreto’s reports, does. Despite this fact, Collingwood and Barreto selectively exclude any CVAP EI RxC analysis from their reports and rely instead on the novel BISG based EI RxC analysis while entirely ignoring the CVAP EI RxC results.

Collingwood and Barreto argue that BISG is a more accurate approach than using CVAP because BISG relies on the actual list of voters that turned out for the election, thereby avoiding the need to estimate turnout.<sup>10</sup> However, there is a clear tradeoff here. BISG must make a probabilistic estimate of each voter’s race/ethnicity and then sum those probabilities to get an estimate of the composition of the voters in each precinct. These estimates of ethnicity are subject to error. In contrast, CVAP does not estimate the race/ethnicity of individuals; it relies instead on the actual self-reported race/ethnicity of each respondent. In turn, using CVAP requires an estimation of turnout, something that is also subject to error. However, unlike the error in the BISG estimation of race/ethnicity, the uncertainty about turnout error is internal to the EI RxC estimation process, and is reflected in the associated confidence intervals. Indeed, providing some empirical measure of that uncertainty in the earlier double regression

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<sup>9</sup> See Grofman & Barreto, *A Reply to Zax*.

<sup>10</sup> B&C Reply at 11.

methodology was much of what the original Grofman and Migalski article sought to achieve by use of the seemingly unrelated regressions (SUR) approach.<sup>11</sup>

The problem is that the EI RxC estimation procedure has no way of knowing that the BISG racial/ethnic turnout numbers are only estimates, with inherent error. Collingwood and Barreto treat BISG as if it is somehow essentially equivalent to the actual voter turnout by race/ethnicity data that is available in states like Georgia, where voters self-report race/ethnicity when they register to vote. And indeed, from the point of view of the EI RxC procedure they appear identical, as both are simply input as the precinct level totals of the raw numbers of actual voters in each of the racial/ethnic categories. This means that while a CVAP based EI RxC incorporates information about the uncertainty of its turnout estimation into its confidence intervals, the BISG based EI RxC excludes any uncertainty inherent in the surname-based estimation of race/ethnicity, and instead reports confidence intervals that only reflect the uncertainty inherent in the ecological inference itself.

The consequence of this is that the confidence intervals from the BISG based EI analysis should be viewed as theoretical minimum intervals that reflect the level of uncertainty in the point estimates that would be appropriate if race/ethnicity was available in self-report at the voter level. Any error or uncertainty inherent in the BISG estimation procedure would be expected to widen those confidence intervals, with the impact rising as the inherent error levels increased. A procedure like Spanish surname list coding, which is widely used in California and Texas, would be at the lower end in terms of this impact on confidence intervals, as at least in those two states the surname list procedure has been shown to have very low error rates. The same cannot be

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<sup>11</sup> Bernard Grofman & Michael Migalski, *Estimating the Extent of Racially Polarized Voting in Multicandidate Elections*, 16 Sociological Methods & Research 427 (1988).

said of the use of the BISG procedure, particularly in a small area like the East Ramapo District. These and other issues with this particular application are discussed much more extensively in the declaration of Dr. Peter Morrison in this case.

In any case, the appropriate use for a novel technique like BISG would be to employ it alongside other more established methods. In the past, this has been the practice with new methodologies, and has generally resulted in alternative methods providing substantively similar results. Here that is not the case. Collingwood and Barreto do not make this comparison because, as noted above, while they employ CVAP based EI RxC for estimating both white versus non-white voting and turnout, they do not provide any CVAP based EI estimates for the crucial breakout of white, Black, and Hispanic voters. In addition, I have not been able to reproduce the Collingwood and Barreto BISG or Catalist estimates that they report using the scripts and data that they provided,<sup>12</sup> so a comparison requires looking at the CVAP results from my report and the BISG results from their report. The CVAP EI RxC estimates from my analysis do not support a conclusion of Black or Hispanic voter cohesion, with the results for Black voter cohesion notably worse than for Hispanic voter cohesion.<sup>13</sup> This pattern reverses in the BISG RxC results provided by Collingwood and Barreto, where the estimates of Black voter cohesion are notably higher than they are in the CVAP based analysis, and the estimates of Hispanic voter cohesion are notably lower than they are in the CVAP based analysis.<sup>14</sup> Even in the BISG results that they provide as their sole analysis of Black and Hispanic voting patterns, the estimates of Hispanic cohesion fail to achieve standard levels of statistical significance.<sup>15</sup>

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<sup>12</sup> See Alford Report at 19, 22.

<sup>13</sup> *Id.* at 18, 21–23.

<sup>14</sup> *Id.* at 19–22.

<sup>15</sup> *Id.*

## Election Algebra

The BISG analysis is not the only novel technique that Collingwood and Barreto find themselves relying on for the first time in this report. In the section of the rebuttal report on what they have coined as ‘election algebra,’ they continue to misrepresent both the value of this novel technique and the degree to which its assumptions are undisputed facts.

On page 13 they state:

*We first start with the quantities that are known and cannot be disputed, the official results of the Tuesday, May 16, 2017 elections for the East Ramapo Central School District Board of Education. In that election, Freilich received a total of 9,530 votes and Dos Reis received a total of 4,503 votes and 12 votes went to write-in candidates for a total of 14,045 votes cast. Given the higher voter turnout among Whites, as reported in our original expert report, there were 10,428 White votes cast out of the total 14,045, which leaves 3,617 votes cast by non-Whites. These numbers are not disputed. Turning to Dr. Alford’s own vote choice estimates of 87% of Whites voting for Freilich, that results in exactly 9,072 Whites who voted for Freilich ( $10,428 \times .87 = 9,072$ ). Given that Freilich received a total of 9,530 votes and 9,072 came from White voters, the only mathematical result is that Freilich received 458 votes from non-Whites ( $9,530 - 9,072 = 458$ ). Finally, some simple division tells us that the 458 non-White votes for Freilich represent 13% of all non-White votes cast. ( $458 \div 3,617 = .1266$ ).*

The entire “election algebra” analysis proceeds from the false premise that we actually know that “there were 10,428 White votes cast out of the total 14,045, which leaves 3,617 votes cast by non-Whites.” Collingwood and Barreto assert that “these numbers are not disputed” which tends to obscure the fact that these are not exact known quantities, but instead are merely *estimates*. The only *facts* in the paragraph that are “known and cannot be disputed” are the election returns that they begin with. The later assertion that “there were 10,428 White votes cast” is an estimate from a CVAP based EI estimation that Collingwood and Barreto ran separately from the vote choice analysis.

Because the districtwide turnout estimate for whites is only an estimate, it has a confidence interval associated with it. While Collingwood and Barreto discuss the results of varying the vote choice estimate for white voters across its confidence interval, they provide no such treatment of the estimate of white turnout. This omission is significant, because the entire “election algebra” concept relies on treating turnout as a fixed non-conditional variable when, in fact, it is not. Even small adjustments to the estimated white turnout rate along its confidence interval would result in concomitant adjustments to the Black turnout rate and to the Hispanic turnout rate along their respective confidence intervals, any and all of which could alter the “election algebra” analysis dramatically. EI RxC acknowledges and accounts for this dynamic in making its estimates of voter choice—the comparatively crude, oversimplified “election algebra” analysis does not.

Collingwood and Barreto go on to apply a “vote choice estimate of 87% of Whites voting for Freilich” that they took from my reported CVAP based EI RxC analysis, but they do not acknowledge that the CVAP based EI RxC estimation that I performed made its own internal estimates of turnout for whites, as well as Blacks, Hispanics, and others. In addition, the EI RxC analysis employs bounds information at the precinct level for each group, something the ‘election algebra’ approach attempts only for whites and non-whites and only at the district-wide level. The full EI RxC estimation for that election in my report is the result of applying King’s estimation procedure to derive as much information as possible from that election data given the precinct level data. There is simply no justification for replacing the resulting estimations of minority voting patterns with a result cobbled together from a district wide subtraction process, and the attempt to do so suggests a misunderstanding of Professor King’s entire EI project.

Even if we were to accept the ‘election algebra’ as a substitute for an actual EI RxC analysis, the results would not demonstrate what Collingwood and Barreto actually need to demonstrate here. Just because a majority of a combined group of all of the non-white voters favored a candidate does not mean that each of the individual minority groups provided cohesive support for that same candidate. This is particularly true when our assumption about the combined group is based, as it is in the ‘election algebra’ approach, on an aggregate turnout estimate for the majority group, and no turnout estimates at all for the various individual minority groups.

### **Confidence Intervals and Statistical Significance**

In their rebuttal report, Collingwood and Barreto discuss at some length their preference for a different approach to testing whether the BISG estimates for Hispanic cohesion reach traditional social science levels of statistical significance. They begin by stating on page 17 that “Dr. Alford does not perform any statistical test to determine if the vote choice estimates for Latinos are statistically ‘insignificant.’” I’m not sure what they mean by this, but in the social sciences, the standard practice is to test whether a particular estimate is statistically significant, not the reverse. The most widely used standard is the .05 level (the equivalent of a 95% confidence interval), two-tailed. Presumably, Collingwood and Barreto do not dispute this, as they report confidence intervals throughout their reports, and always at the 95% level.

They go on to state that “Dr. Alford claims that it is not possible to arrive at a specific percentage point estimate, such as 80%, but rather one can only think about the full range of the confidence interval and not the specific point estimate”.<sup>16</sup> I do not claim that at all.

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<sup>16</sup> B&C Reply at 19.

All I do is use the 95% confidence intervals that Collingwood and Barreto provide for each of their EI RxC point estimates to ask whether that 95% confidence interval reaches to 50%, a value at which the Hispanic vote would be completely non-cohesive, as Hispanic voters would be perfectly divided in their preference.<sup>17</sup> That is what confidence intervals are for, and my analysis of the confidence intervals is consistent with the standards of social science research.

Similarly, Collingwood and Barreto assert:

*Dr. Alford does not draw any contrary conclusions—he only suggests that it is not possible to draw conclusions based on the available data. For example, we conclude in our September 14 Report (and reaffirm in this report), that there is racially-polarized voting in East Ramapo, in that Black and Latino voters are politically cohesive both within and across groups and their preferred candidates are usually defeated by a large White voting bloc. In contrast, Dr. Alford concludes only that it is not possible to determine whether racially polarized voting exists in East Ramapo, and makes no finding that it does not exist.*<sup>18</sup>

This is simply not true. Again, the most important finding of my report is that there is clear evidence here, whether based on my election analysis or that of Collingwood and Barreto, that voting behavior in District elections is driven by policy divides rather than by the race or ethnicity of the candidates. The voting patterns in the elections examined here are characterized by policy polarization and not indicative of racial polarized voting.

The critique mounted by Collingwood and Barreto offers two alternatives for computing the probability that Hispanics did not support the candidate that the point estimate would suggest. On pages 20–21 they offer this analysis:

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<sup>17</sup> Alford Report at 22.

<sup>18</sup> B&C Reply at 3.

*[W]e have calculated the confidence interval for the difference, and the probability that the difference between the Morales and Rothman vote shares among Latinos is not zero—meaning there is a real statistically significant difference. The key question here is whether or not the difference is 0 (meaning they are the same) or if we can reverse the expectation (i.e., “did Rothman win more Latino votes than Morales?”). The question of interest is the value of  $P(\lambda_{LRothman} - \lambda_{LMorales} > 0)$ . This is estimated directly by calculating the proportion of draws from the posterior distribution for which this inequality holds. We can graph the distribution of this difference, which is especially informative, since we may examine not just whether Morales is likely preferred, but also by what degree. The estimate is  $P(\lambda_L - \lambda_R > 0) \approx 0.178$ , just under 18% probability that Latino voters preferred Rothman to Morales in 2015. In fact, the probability of Latino voters favoring Morales by more than 10 pts is 0.606, around 61%. The median value of our posterior distribution over  $\lambda_{diff} = \lambda_{LMorales} - \lambda_{LRothman}$  is very far from zero; in fact, it is a quite robust 0.131 (and mean = 0.121).*

*What we are showing here is that the evidence is far more tilted to demonstrating polarized voting, than not. The most likely scenario is that Morales was favored by a margin of 13% among Latinos casting votes. The chance that Morales actually did not win more Latino votes than Rothman is about the same as the chance of rolling a six on one toss of a fair six-sided die, while the chance that Morales won more Latino votes is akin to the chance of rolling a one, two, three, four, or five on a single toss of the die.*

I don’t have any disagreement with this approach or this calculation. As they make quite clear, they estimate that the chance that Hispanic voters either were divided 50/50 in their preference for Rothman or Morales (perfectly non-cohesive) or actually preferred the same candidate (Rothman) as did whites is “just under 18%.” While this may not seem large in comparison to the just over 82% chance that Hispanics prefer Morales, that is not the point of the social science standard of a 95% confidence interval, or the equivalent .05 level of statistical significance. Clearly .18 is well above .05, indicating that we clearly do find that there is a greater than 5% chance that our conclusion that Morales is the preferred candidate is wrong. Put another way, it is clear that 72% falls well below the 95% threshold. The social science

standard is in effect based on a twenty-sided die. While that may not suit Collingwood and Barreto in this instance, it is not my standard; it is the social science standard that we all observe in our published research.

In the remainder of their discussion of statistical significance, Collingwood and Barreto assert that “[i]n addition to assessing if the 95% confidence interval of one candidate (Morales), overlaps with the upper bound of the estimate of the second candidate (Rothman), Dr. Alford incorrectly compares how both confidence intervals overlap.”<sup>19</sup> That is simply untrue. As I indicated above, I am not comparing how two confidence intervals overlap. I am assessing whether one confidence interval includes the critical value of 50% support for a given candidate.

Collingwood and Barreto go on to say that I assert that I “would not be able to conclude” that there is a difference between the Latino support for Morales and Rothman.<sup>20</sup> That, too, is simply incorrect. Instead, I said that, regarding a hypothetical example of Black voter cohesion, “if the confidence interval for the same 55% point estimate ranged between 25% and 85%, then one would not be able to conclude *at the standard political science level of 95% confidence* that candidate ‘A’ received a majority of the Black vote, since multiple possible values at or below 50% lie within the 95% confidence interval.”<sup>21</sup>

In sum, Collingwood and Barreto acknowledge that 95% confidence levels are the standard in the social sciences, and those are the confidence intervals that they provide. While Collingwood and Barreto might prefer a more relaxed test of significance, that academic debate is not appropriately considered in the context of a Voting Rights Act case.

### **Which Candidates Are Relevant**

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<sup>19</sup> *Id.* at 23.

<sup>20</sup> *Id.* at 24.

<sup>21</sup> Alford Report at 10 (emphasis added).

Collingwood and Barreto again mischaracterize my opinion when they assert:

*Since Gingles itself, the Supreme Court has expressly rejected a key assumption underlying much of Dr. Alford's report, namely, that the District's at-large system should be immunized from liability because a White voting bloc has engineered the election of a few minority candidates—namely, Bernard Charles, Pierre Germain, Maraluz Corado, and Juan Pablo Ramirez.<sup>22</sup>*

At no point in my reports do I assert anything of the sort. As I discussed above, the central conclusion of my analysis of elections in the case is that the stable levels of support for candidates across these elections is a function of a long-standing policy polarization over property tax rates and public spending for the benefit of private school and public school students, and not a pattern of racially polarized voting.

It is not so much the success of these minority candidates that matters for purposes of my analysis, it is the lack of any evidence that voters' support for any of the minority candidates, whether winners or losers, is conditional on the race or ethnicity of the candidate. This shows that any degree of polarization in the electorate is not likely attributable to racial concerns. If racial concerns were a motivating factor for voters, even in some small proportion, it would likely be evident in a detectable variation in the degree of support of white voters for minority candidates versus white candidates. There is no evidence of any such variation here.

I don't dispute that Bernard Charles, Pierre Germain, Maraluz Corado, and Juan Pablo Ramirez were the preferred candidates of the majority white voting population. Nor do I dispute that this population consists of many residents of the District's Orthodox Jewish community, which tends to resist increasing property taxes and to support provision of educational services that benefit private school students and public school students equally, such as bussing,

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<sup>22</sup> B&C Reply at 26.

textbooks, and special education services. I do dispute Collingwood and Barreto’s conclusory assumption that these candidates were not *also* the preferred candidates of the District’s Black voters, since there is no reliable evidence from which they can make such a conclusion.

I also dispute Collingwood and Barreto’s apparent assumption that the fact that these minority candidates obtained the support of the District’s Orthodox Jewish community constitutes some sort of “engineering” of the District’s elections to “immunize” the District’s at-large elections from liability under the Voting Rights Act. Candidates that support political positions that are most popular with the majority of voters and who obtain the support of influential members of the community typically are elected over candidates that take political positions that are unpopular with the majority of voters and that do not seek such endorsements.

### **Senate Factors**

In their rebuttal report Collingwood and Barreto continue to focus on characteristics of the election system that may work to enhance the discriminatory effect of the at-large election system. They do this based largely on national patterns while avoiding entirely any discussion of the role of socioeconomic factors in the District.<sup>23</sup> Such socioeconomic factors are typically present and typically cited as having both a direct effect on reducing minority participation and a role in conditioning the impact of other enhancing factors. The importance of this here is that the pattern of socioeconomic disparity is highly unusual in East Ramapo, since the resource disparity that typically disadvantages minorities is largely reversed here.<sup>24</sup> National studies that connect these enhancing factors to minority participation reflect that nationally, and indeed in most localities as well, the pattern of resource disparity disadvantages minority voters.

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<sup>23</sup> See B&C Reply at 27–29.

<sup>24</sup> See Alford Report at 29–30; Cooper Report at 20–21.

Factors that interact with resource disparity to reduce access to the electoral system, such as off-cycle elections or a small number of polling places, cannot simply be assumed to have a disproportionate negative effect on minority voters when, as we see here, the minority population *does not* suffer from any resource disparity.

The issue of a slating process is also addressed again in the rebuttal report. As Collingwood and Barreto note on page 39 of the report, “Mr. Charles’ testimony strongly indicates that the only minority candidates who can get elected to the Board are those who are acceptable to the District’s White, Orthodox and Hasidic Jewish voters and that those voters will not be amenable to electing minority candidates if those minority candidates oppose the interests of the private school community.” This simply makes the point that I believe is crucial to this entire case. Like the voting patterns themselves, the “slating” issue, on both the private school and public school side, appears to be concerned exclusively with the policy positions of candidates and not with the race or ethnicity of the candidates. Indeed, there is no suggestion that leaders from either side in this long-running policy dispute were willing to endorse *any* candidate, white or minority, if those candidates disagreed with their policy preferences.

What Mr. Charles and Mr. Germain describe is a process in which, rather than being recruited for their supposedly “safe” status, they decided to run on their own and then sought the endorsements of influential members of the Orthodox Jewish community. As Mr. Charles indicated, that required supporting certain private school-related policy positions, and as he concludes, the failure to support those policy positions would have resulted in losing the support of the Orthodox Jewish community. This sounds like the way election politics typically works. Candidates that are elected typically espouse policy positions that are popular with the majority

of voters. A candidate that adopts and espouses unpopular political and policy positions typically loses. These forces are in play whether the candidate is white, Black, or Hispanic.

That is exactly the process described by Mr. Charles and Mr. Germaine. They campaigned in the Orthodox Jewish community and sought the support of influential members of that community. That is what successful politicians typically do. They were not rebuffed on the basis of their race or ethnicity, but were endorsed and supported by the Orthodox Jewish community, and believed that this endorsement was conditioned on their support for politically popular education and tax policies.

On both the private school and public school sides of this policy debate, white, Black, and Hispanic candidates have been endorsed by *policy* advocacy groups, and subsequently been supported by voters that shared those *policy* preferences. Collingwood and Barreto provide no evidence that either the race or ethnicity of the candidates, or the race or ethnicity of the voters, has played a role in either the endorsement of, or the level of voter support for, the candidates.

In footnote 71 on page 33 of their rebuttal report Collingwood and Barreto assert that “[i]n offering his opinion on ‘public school’ and ‘private school’ factions, Dr. Alford purportedly reviewed sections of Plaintiffs’ complaint (Alford Report, p. 27, n.35), but conspicuously ignores racial demographic data in those sections showing District public school students to be almost exclusively racial minorities and the private school students to be almost exclusively White.” I am not ignoring that reality. It is precisely that reality that elevates the importance of carefully examining the role of race and ethnicity in the patterns of candidate endorsements and critically examining the patterns of voting behavior, so that a legitimate public policy debate is not mistaken for evidence of a racial debate.

When we have a case, as we do here, where racial and ethnic divides may correlate with a major public policy divide, the impact of the race and ethnicity of candidates is crucial in helping to distinguish situations in which the dispute is genuinely one of policy disagreement, from situations in which the underlying voter division is driven by racial or ethnic considerations. Here, no one disagrees that the District's voters are divided along policy lines.

Pursuant to 28 U.S.C. § 1746, I declare under penalty of perjury that the foregoing is true and correct.

Executed on December 7, 2018



John R. Alford